



STANFORD UNIVERSITY MEDICAL CENTER

STANFORD, CALIFORNIA 94305

January 27, 1975

STANFORD UNIVERSITY SCHOOL OF MEDICINE
Department of Genetics
(415) 497-5052

Mr. William Gorham
President
The Urban Institute
2100 M Street, N.W.
Washington, D.C. 20037

Dear Bill,

Thank you for sending me Dr. Sowell's manuscript. It was not only a privilege to read it, it was a pleasure. It is a subject I often approach with dread on account of the level of polemical nonsense and it was a delight to see a treatment that stuck close to facts and to the clear presentation of an analytical methodology.

You should then assess my few critical remarks in the light of global admiration. Certainly it deserves to be published under your imprint. I do have a few suggestions that you may or may not wish to take into account.

The treatment of temporal change in I.Q. measurement is masterful. I have seen many other allusions to the studies on immigrant groups* but I have never seen the actual numbers derived from contemporary measurements put down as they are here nor with the kind of critical discussion that they deserve and that Sowell rightfully gives them. One feels one hardly needs go any further in discussions of the central thesis!

I do have to offer some comment with respect to the general theme of sex difference in the way that Sowell uses it. While it is indeed true, as Sowell introduces the subject on page 7, that certain traits among females are more buffered against environmental variation, I would hesitate very strongly to elevate this to a general principal that could be used in the reverse sense for syllogistic reasoning. Differential heritability by sex needs to be taken sui generis, case by case, and for a situation as complex as development of intelligence very specific interactions of culture and gender can hardly be ignored. Not to mention a whole range of issues that would get us into sex discrimination policy discussions, the generally more rapid intellectual maturation of girls, and their perceived greater co-operativeness in school situations where discipline is a great problem, would a priori be expected to confuse the findings.

On the other hand, the empirical data on difference in variance by sex should not be ignored but could well be put as requiring a plausible explanation in terms of the respective models. I do not think there is a strong a priori case along the lines implied by the statement under sex difference on page 7. In any event, to the extent that Sowell wishes to

LT. J. P. KENNEDY, JR. LABORATORIES FOR MOLECULAR MEDICINE, DEDICATED TO RESEARCH IN MENTAL RETARDATION

MOLECULAR BIOLOGY

HEREDITY

NEUROBIOLOGY

DEVELOPMENTAL MEDICINE

over

persevere with this argument, I think he should document the other attributes which have been found to show the response that he implies.

My other main criticism is perhaps more one of statement than of analysis, but I thought the general discussion on page 5 might be challenged or might be readily misunderstood. Perhaps you want to try out just this one presentation on a number of other people to be sure my own responses are not idiosyncratic. I have no quarrel at all, of course, with the general concepts of the I.Q. plane. In the end one must make some statement to the effect that $I = f(h,e)$. I am rather less happy with the general notion that either h or e can be described by a single metric - that is that one can ignore terms that relate to very specific interactions between particular genes and particular environment. (An outstanding example would be the needs for special education, not merely better education in general, to respond to the needs of the congenitally deaf so as to raise their I.Q. from 0 to 100). My own prejudices about this general problem would lead me to give much more weight to such issues of specific interaction without suggesting that they are necessarily very much more cogent in discussions as between races than they are within them. However, we may be discussing a culture context in which relatively rather more is being demanded of the schools than of the home environment in which such specificities are the most likely to be recognized and dealt with, without the benefit of external social policies.

But what I think is either left out or not clearly stated is that the problem of heredity versus environment is not whether there exists such a plane with extreme points of the obvious kind that Sowell correctly and clearly articulates; but rather the actual distribution in a given population of the inputs that are recognized as the values along the h and the e axes.

At the bottom of page 5 Sowell asserts that the statement that intelligence is due y percent to heredity and z percent to environment is not meaningful." I would have said that it has a very definite meaning which is very often misunderstood; that the attribution of heritability is one that attaches not to the trait "intelligence" but to the population which then contains its own characteristic distribution of the h and e factors that result in a manifest level of intelligence. I am not sure whether we are here discussing a difference in verbal taste or a more fundamental conceptual discrepancy.

My further comments are more by way of picking at rather small nits but I hope you think them more beneficial than otherwise.

The statement on the very first page that the question is ultimately an empirical one, independent of anyone's beliefs, hopes or fears, assumes that we are not in fact also bedeviled by semantic confusion. From the wide range of current controversies we can extract a meaningful issue and this is the one that Sowell then further addresses. I am raising essentially the same issue in commenting on page 4 where there is an assertion about the heredity-environment controversy. There are, in fact, very many such controversies and it is only the better defined of them that will lend themselves to the further statements that, for example, "it is well understood that intelligence is a result of both heredity and environment" and so forth.

over

At page 6 the linear model is in fact very close to what Jensen has asserted and Shockley assumed. But perhaps I am responding to it mainly in terms of my own argument about specificity.

There were some pages I particularly enjoyed, like page 21!

Page 25-26. I guess I would have made a somewhat weaker theorem which is still devastating and run the argument in the other direction to some extent. A demonstration of dramatic shifts with time in other ethnic groups, not plausibly explained at all by genetic change, is a devastating attack on the invariance of I.Q. with respect to other social situations. Whether or not the social pathology of the European immigrants can be compared with that of our black American populations is another question that really did not have to be addressed. For the very rapid progress of the Jewish immigrants one can scarcely forget that for thousands of years no Jewish group has ever accepted the imputation of inferiority no matter how brutally this was thrust upon them. (And particularly in the intellectual sphere!) It would take us needlessly far afield to go into the specific elements of social pathology that are related to retardation in I.Q. measurement. One cannot but note a high degree of correlation between cultural convergence and the convergence of the I.Q. scores. But this is not the place to go into the more general aspects of the arguments about meritocracy as economic utility.

Page 26 in re sex differences. I think Jensen could cope with such data ad hoc. It would be rather easy to invent perhaps somewhat credibly genes that specifically interact with sex to account for any result that may be found.

Page 28. For Sephardic Jews in Israel I would qualify with the term Oriental.

If this is to be the definitive publication, would it be possible to derive measures of variance as well as mean for use in the various tables on I.Q. of different groups?

42b makes a very cogent point. My copy was missing page 63, so I have no hint as to reference 59a. I have often wondered about the representiveness of samples that purported to describe the "average black population".

Page 55-56. My previous remarks about the analysis based on sex difference apply here as well. The term inescapable corollary is especially at issue.

Page 60, reference 11. Is American "Indian" still an acceptable epithet?

Appendix: Research Methods. I am interested that you do not use the census occupational classification. You might wish to discuss some of the (possibly excellent) reasons not to. I know that the census would classify a number of the people under number 7 as professionals. But who is a writer, artist or other profession of this nature? Does that include you and me? On matters that have to do with educational performance, I would have thought it would be particularly interesting to separate out such categories as school teachers and librarians, writers, journalists, etc. with the implied

1/27/75

Preoccupation with verbal skills that they reflect. In retrospective summary I guess I would recommend that the discussion and material on sex difference be abbreviated for the purpose of the present paper at least on the basis of my belief that the theoretical premise has been overstated. On the other hand, it deserves to be spelled out in considerable detail as a separate rather more technical document where the theoretical issues can be discussed at greater length and at a more technical level.

The material on temporal change is manifestly beyond challenge on such theoretical grounds although I confess I have not spent a great deal of time trying to think of possible criticisms. Most of the distortions - attempts at assimilation in particular - would tend to work in the opposite direction although I have no empirical data to support this. (I have in mind those Jews who attempted to anglicize their names and whether they are an unbiased sample with respect to intelligence or schooling - perhaps they are even selected on the basis of some difficulty in competition rather than the converse). One can think of analogs of course for other ethnic groups. Akin to that would be the ambiguities raised by inter-marriage, about which exactly the same questions could be raised.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr

- * P.S. There is an excellent exposition on "The Mental Testing Controversy: Race and Nationality, 1919-1930" in the Ph.D. dissertation by H. Cravens, 1969, American Scientists and the Heredity Environment Controversy 1883-1940 (available from University Microfilms, 69-21678). Cravens points out that Brigham recanted his earlier racist views (see Psychological Review 37:164, 1930). Cravens also cites the "Last Review of Racial Psychology" as the termination of the field for that interval: Pinchmer, Intelligence Tests Psychological Bulletin 32:453, 1935. Perhaps the same material is covered in reference 75 in Sowell's paper.

and see also Kamin's angry polemic (attached).